Fw: Review of EPA National Technical Workgroup Draft Seismicity Report

Susie McKenzie to: dellinger.philip, dorsey.nancy, Ken-E Johnson, graves.brian, Rob Lawrence, Keara Moore 01/29/2013 08:41 AM

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1 attachment

Induced-Seismicity_Review_BS.pdf

Combined comments from Brian Stump and Chris Hayward, SMU

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From: "Brian W. Stump" <bstump@smu.edu>

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Cc: Chris Hayward <hayward@smu.edu>

Date: 01/28/2013 09:21 PM

Subject: Review of EPA National Technical Workgroup Draft Seismicity Report

Dear Susie McKenzie,

We are forwarding to you our review comments on the draft report, "Minimizing and Managing Potential Impacts of the Induced-Seismicity from Class II Disposal Wells: Practical Approaches." We have provided an integrated set of comments based on our review of the report.

We both found that the report provided practical insights into problems related to induced seismology. In particular, it does a good job of articulating the need for an inter disciplinary approach to solving this important issue involving expertise from seismology to petroleum engineering. Moving forward will require open and frank exchanges of data and expertise.

Our comments are intended for your consideration highlighting possible areas of either improvements or enhancements to the report that might increase its impact. We both felt that with the publication of the recent National of academy of Sciences Report consideration might be given to explicitly discussing the similarities and differences in the conclusions of the two reports.

If either of us can be of further help, please feel free to contact us.

Best Regards,

Brian Stump and Chris Hayward

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Comments on "Minimizing and Managing Potential Impacts of Induced-Seismicity from Class II Disposal Wells: Practical Approaches, Draft November 27, 2012.

Brian W Stump & Chris Hayward
Southern Methodist University
28 January 2012

1. Page 2, Lines 2-3. Although mentioned latter in the report, temperature and chemical effects associated with injected fluids as mechanisms for induced seismicity are possible but in light of this current study not probable.
2. Page 2, Lines 8-9. It is important to note that proof of induced seismicity is difficult to achieve. I agree that it is not a prerequisite for prudent action. I think it is worth adding at this point that a single coherent physical model of the process does not exist but a collaborative program to improve the model would be of benefit in addressing these problems.
3. Page 2, Lines 16-19. It might be worth noting that this better understanding will come about by collaborative work between a wide variety of individuals in industry, government and research. This is particularly the case for combining earthquake seismology, a field with theory developed principally in academia with observations and operations by civil authorities with combinations of reservoir engineering and exploration geology and geophysics developed principally in industry.
4. Page 3, third paragraph. It is worth adding that the sequestration of CO_2 underground could add another source human activity-induced earthquakes.
5. Page 3, Footnote 6. This report does not otherwise mention the hazards to USDW and some of us are left wondering how those hazards are defined and whether examples of such hazards might be given. For example, would widespread sanding of existing DW wells be considered a hazard or disturbance of filter beds in water processing plants or is it just contamination of existing underground potable water? If the later, is there an example – or an example from natural EQ occurrence?
6. Page 4, Lines 2-4. It would be good to add references supporting the contention that enhanced recovery projects generally pose less potential to induced seismicity. Although the physical argument in terms of fluid balance is a good one I am not sure that the data of such induced earthquakes related to enhanced recovery supports the broad nature of this statement. For example, Cogdell field in West Texas. One might also consider the Geysers where additional water is injected to balance the fluid loss – yet still this is a major area of induced earthquakes.
7. Page 4-7 Stressed Faults. Since the current understanding is that the earth is critically stressed everywhere, is the implication that the stressed faults are all those faults that are favorably oriented?
8. Page 8, second paragraph. In discussing the geoscience factors related to injection-induced seismicity regional factors are reviewed in this section.

One point that is not well developed is the orientation of the current *in situ* stress field with faults. It is worth noting that in cases where the current *in situ* stress field is optimally oriented with old or inactive faults that there might be the opportunity for inducing earthquakes along these features. I believe that this is mentioned in the USGS appendix but does not seem to have found a place in the body of the report. This association emphasizes the need to characterize both the active and inactive faults in the region as well as their geometry relative to the current *in situ* stress field.

9. Page 8. The lack of --small-- events in the historical seismic record may be due to a lack of seismometers, sparse population, and a low natural recurrence rate coupled with a short recording history. Large events (M7) would be recorded in the historical record and possibly in the paleoseismic record.
10. Page 9. Wells and Coopersmith's data set was limited below M5. The implication in the paragraph is that there is a well understood or observed relationship between 7.1 and -4. It may be that the relationships observed for moderate to large earthquake do not scale down to magnitude 2.
11. Page 10. In view of the Cleburne and DFW experience where magnitude 2.0 and below events generated felt reports (as well as news activity) one may want to consider adding a depth dependence to this felt scale. Shallow events and events in the US Northeast have different thresholds.
12. Page 11. A citation or quantification would help demonstrate that the total energy difference for injections accompanying a small geothermal project is much less than the total energy from a large fluid disposal program.
13. Page 11. HF events. It may be worth mentioning the Blackpool and Horn River Basin HF events
14. Page 12. What is the currently acceptable proof? Based on this and other reports, it seems that the Rocky Mountain Arsenal is regarded as a study where there was proof, and the implication may be that in order to prove that a particular well is actively inducing seismicity it is necessary to show a relation between the modulation of injection activity and the modulation in earthquake activity, a particularly difficult study to find public acceptance in areas of significant population.
15. Page 13. How os the radius of 5-12 miles selected? Is it based partly on the assumed accuracy associated with the hypocenters as well as the maximum expected radius for fluid migration?
16. Page 14. Based on our current data (M 3 events continuing) as well as the Eisner report (now published), can we still say that the frequency and magnitude of the events are statistically significantly reduced?
17. Page 18. The second lesson learned is that an improved understanding of reservoir behavior in the disposal zone will lead to better characterization of induced seismicity. In general I agree with this statement but did not find quantitative evidence to support this conclusion in the case studies except in the study showing relatively long distance communication between adjacent wells.

18. Page 18. The forth lesson learned discusses the importance of increased seismic monitoring to improve earthquake locations. I think this argument can be quantified based on the USGS appendix and that some characteristics numbers including illustrating the large errors typical in regional locations provided by USGS. I think there is an underlying issue that is not discussed here or in the Appendix. Even with close-in stations there will be tradeoffs in the estimate of event depth and the assumed P and S wave velocity model used for the location. As a result, depth will be one of the hardest parameters to estimate even with local instrumentation. There should be some recognition of this fact in the report.
19. Page 19, Lines 31-32. I would add the orientation of the *in situ* stress field relative to the existing faults to this list. Possibly the use if stressed fault is meant to convey this point but explicitly including the relationship between the stress field and fault orientation would strengthen the description.
20. Page 22, Annular pressure tests and production logging.. Is there an example where this was done after seismic activity?
21. Page 24, Existing disposal well. While it is brought out later in the study, it wasn't obvious to us at this point that this was being considered from a regional perspective. For example the older Cleburne injector was permitted well before the additional activity in the region.
22. Page 25. In the proposed decision model, one of the considerations is "Have there been regional seismic event." It is critical to define what is meant by regional relative to the proposed well, especially in light of relatively large uncertainties in earthquake locations using regional stations.
23. Page 27, Lines 18-19. I strongly endorse this statement. It is critical that multi-disciplinary approaches be implemented in order to better understand induced earthquakes. This approach includes the free exchange and sharing of databases, models and interpretations. Such an approach will provide the basis for identifying outstanding issues that might be illuminated with additional work or data. Within a research framework this cooperation provides a path forward for a better physical understanding of the processes and the development of a set of coherent best practices.
24. Page 30. In the case of geothermal seismic activity there is a strong public component associated with the assessment of the impact. Although this is mentioned in passing later in this report, there is no discussion of the same level of public involvement in the USDW case. Should there be some mechanism to increase public involvement and education?
25. Page 32, .. with early monitoring it may be possible to reduce... One may want to consider the statements in the NAS report " No capability to predict how reducing volumes, rates, and pressures will affect seismicity once started" and "evaluating .. is difficult because there is no cost effective way to locate unmapped faults and measure *in situ* stress." If this report differs from the NAS, it would be useful to point out the reasoning, or if there is no disagreement perhaps to remark that this report accepts or agrees with the NAS...

26. Page 35, Position uncertainty. While a later appendix points out that 'many parts of the world' includes large parts of the US, this may be worth pointing out here as well.
27. Page 35, Damage. Damage is relative and dependent on construction practices, regional and local geology, earthquake depth, and geologic and cultural hazards. The included table may lead one to consider that any earthquake under M5 could be ignored. From a public perspective this is not the case, since the Soultz France project was ended due to possible damage to structures from a M 2.9 earthquake.
28. Page 36, Term stressed fault. Is that an accepted term with a citation in the literature or is this a first use here?
29. Appendix B. Should the estimated error in location accuracy (which will vary with time) be used to select the area around the well? Seismic station spacing in the US is as large as 200 miles. It might be more useful to have a accuracy related to that rather than the 100 miles. Also, it would be useful to include the lack of depth accuracy since this has often been used in news and industry arguments as to the cause of the earthquake.
30. Appendix C is very useful and provides insight to petroleum engineering considerations that can be understood by the non-specialist. There needs to be a companion appendix that discusses seismological practices for the non-specialists that provides some understanding of earthquake location, the associated errors, estimates of earthquake size (magnitude and moment) and earthquake source characterization such as fault orientation, stress drops, and fault size. The report by the USGS in Appendix M addresses specific questions that were posed to them and as such provide useful information. The Appendix I am envisioning is more basic discussing some of the fundamental measurements and estimates that come out of seismological data. The reason for inclusion in the report is to high light the importance of bringing together data and techniques from different disciplines in order to better address questions related to induced earthquakes and the use of existing data to understand regional seismicity. Appendix M also makes some generic suggestions such as preinstalling a local network prior to injection, measuring *in situ* stress, but does not suggest enough specifics to indicate how much of a burden this might be. For example, roughly what range of recording time and detection thresholds might be needed? What is the cost relative to injection operations? How would *in situ* stress be measured and on what spacing?
31. Appendix D and following. The earthquakes on the maps are difficult to distinguish from the numerous red dots of gas wells. Perhaps a different colored symbol could be used. In addition to the epicenters shown on the map, one may want to add the formal error ellipse to indicate the uncertainty associated with the events.
32. Page D-4, lines 7-9. The work by Eisner on the DFW Airport events has now been published. A second independent group has reanalyzed the data and published a paper as well. References below:

a. Janska, Eva and Leo Eisner (2012). Ongoing seismicity in the Dallas-Fort Worth area, The Leading Edge 31, 12(2012);pp. 1462-1468.
b. Reiter, Delaine, Mark Leidig, Seung-Hoon Yoo and Kevin Mayeda (2012). Source characteristics of seismicity associated with underground wastewater disposal: A case study from the Dallas-Fort Worth earthquake sequence, The Leading Edge 31, 12(2012);pp. 1454-1460.

33. Page D-6, lines 30 and 31. Analysis of the data from the Cleburne earthquake sequence is now complete. The referenced paper was submitted to the Bulletin of the Seismological Society of America in the fall of 2012. The review is anticipated in the first quarter of 2013.